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A FEW WORDS ON

HOMŒOPATHY

AND

HOMŒOPATHIC HOSPITALS

CHIEFLY IN

REPLY TO PROFESSOR HENDERSON

BEING A SEQUEL TO THE "EDINBURGH ESSAY" ON HOMŒOPATHY.

BY


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TO THE EDITOR OF THE BRITISH JOURNAL OF HOMŒOPATHY.

SIR—I have just received and read the fifty-two pages of clever criticism in your number for April 1857, in which you and Dr. Henderson reply to my thirty-seven pages on Homœopathy in the *Edinburgh Essays*. If the controversy thus raised were to go on expanding in a similar proportion, I fear we should soon be as far beyond the patience of our readers as you are already above my comprehension. I can truly say, that after perusing your elaborate explanations, I have ended in a most complete state of confusion and uncertainty as to what the great therapeutic principle of Homœopathy, about which we have heard so much, really is. You say that the principle in question is not *universal*, only *general*. I must say, that I fail to perceive the difference; but you go on to make admissions which seem to tend to the view that it is neither the one nor the other, but loop-holed with exceptions, limitations, and contradictions, in every direction in which it is possible to bring it into view. Is such a principle as this worth fighting for? Or rather, when fought for and won, what will it amount to? Here it is, for example, in the case of quinine, the only case in which you argue the point fully against me. You say that if quinine is “given long and perseveringly to scores, or even hundreds of individuals” (see p. 294), it may be expected to develop in one or two persons, possessed of a certain “specific susceptibility,” and *only* in these persons, a certain “specific action;” in all the rest it will present its ordinary action, having no relation to its well-known curative effects in ague;

but in the small and exceptional number it will present a “specific action,” having to its curative action in ague—a resemblance?—Yes, a resemblance, “but a resemblance so faint and indistinct, as to be difficult to recognize without great care and trouble” (see p. 293). You may safely defy anybody to disprove this position, and I certainly shall not attempt to do so. Like the cuttle-fish, you are safe in your own ink. But is the homœopathic “general” law to rest upon such instances as this, which you seem to have selected as the plainest and most manageable of my examples? I chose my examples, because they had been chosen before me by Hahnemann himself, as the best battle-ground for Homœopathy. Yet two of my instances you dismiss as being almost irrelevant, and the third you dispose of as I have indicated above. It was the undoubted curative action of Peruvian bark in ague which, it is said, first suggested Homœopathy to Hahnemann in 1790. Surely there ought to have been time to make this part of the subject, at least, clear enough since then! Yet in 1857 we find you struggling with mystifications which, to my humble apprehension, seem to be nothing but an endeavour to conceal the entire deficiency of trustworthy evidence on the subject; or rather to raise a cloud of words about a fact which multiplied experiments have made sufficiently clear to unprejudiced people, viz., that the law “*similia similibus*” cannot, by any legitimate expenditure of verbal logic, be extracted from this, the first and most notable of its alleged illustrations. It is as if the fall of the apple should turn out to have no real relation to the law of gravitation!

My object in writing now, however, is not to assist you in “darkening counsel by words without knowledge,” but simply to answer a challenge of Professor Henderson. Dr. Fleischmann of Vienna, the author of some too celebrated hospital statistics, has, it appears, had his veracity impugned by me in a way unusual among gentlemen, and particularly displeasing to Dr. Henderson. I am called upon to prove certain allegations “by evidence,” or, failing that, to make “an ample apology” for traducing Dr. Fleischmann. Dr. Henderson rather hesitatingly presumes that I would not intentionally commit “a gross violation of truth and justice.” I thank

him even for this very moderate good opinion, on such a point. In all that concerns my arguments, Professor Henderson may be scornful or severe, just as it pleases him, and he will find me very callous; but on this one point I confess I am sensitive, because I do not believe Dr. Henderson to be anything worse than a man hopelessly involved in the mud of a hopeless position—a very Slough of Despond, which he got into, it is true, foolishly; but where, being in, he has fought with an energy, a determination, and (pardon the expression) a thoroughly British pluck worthy of a better cause. I did not, certainly, write to please Dr. Henderson; but no man whom I care to call a gentleman, shall seek a gentleman's explanation from me without finding a ready response.

There are various ways of producing the effect of a *lie*. I am really sorry to use so ugly a word; but it is this very ugly word, and no other, which Professor Henderson has chosen, to designate the construction I have put upon the statistics of the Homœopathic Hospital at Vienna. A lie is a perversion of the truth; but its being a lie depends not only on the fact of perversion, but on the amount and clearness of the consciousness which the perverter had of what he was doing. Now, I certainly believe, because I have proved it to demonstration, that a perversion of the truth did take place in the case of the Vienna Homœopathic Hospital:—viz., that that hospital was made to appear to contain an enormous excess of severe acute diseases above the possible proportion of such diseases in such a hospital population. I have also proved, in a manner not to be gainsayed, that even according to the published returns of that hospital, curable and trivial diseases singularly abound in it, while incurable and organic diseases are curiously and suspiciously deficient. I also came to the conclusion, that this was not the result of accident, but of a plan by which curable cases were chosen and incurable ones excluded; and I saw the results of this plan in the shouts of triumph with which the publication of Fleischmann's statistics was hailed by the homœopaths all over Europe. What a wonderful hospital, said they, in which the total mortality of diseases is only $6\frac{1}{4}$ per cent, and in which the mortality of the severest acute diseases is reduced to an inconceivably small fraction! To

both of these allegations I had my answer ;* and it was as follows :—

To the first allegation, the small mortality of the hospital, I answered that it would have been very dreadful indeed, if in a hospital in which sore throat and shingles, catarrh, dyspepsia, colic, headache, etc. etc., were predominating diseases, the mortality had been otherwise than very small. But I further answered, that *it was not very small* ; not so small, for instance, as in fully one half of the general hospitals of Scotland, and not nearly so small as in most of the provincial hospitals in England. And I further remarked, that I doubted if there was any hospital in England or Scotland which admitted 5 per cent of cases of common sore-throat or cynanche tonsillaris, like this Homœopathic Hospital. Moreover, I thought that for the Homœopathic Hospital, under these circumstances, $6\frac{1}{4}$ per cent was an enormous mortality, considering that, in Canterbury, the hospital mortality is 2 per cent ; in Cambridge, 2.4 per cent ; and in Exeter, 3.2 per cent, under the ordinary methods, and, it is to be presumed, without any selection of cases.

To the second allegation, the small mortality of severe acute diseases in the Homœopathic Hospital, I answered that the whole question was, *whether those diseases were really severe*. I stated that the mere names given to them was no proof of their having been severe diseases, because every one acquainted with disease knows that the names given to its different forms are, to a certain extent, arbitrary ; and that it is very possible to increase or diminish the number of cases of pneumonia, pleurisy, peritonitis, pericarditis, etc., as you choose to interpret certain symptoms and physical signs, which have a grave or a trivial significance according to the form and manner of their correlation. I further stated, that the enormous and incredible number of these diseases in the Homœopathic returns, was a proof to every reasonable mind that Dr. Fleischmann's mode of viewing this subject was different from the common one ; that his pneumonia was not our pneumonia, his pericarditis not our pericarditis, and so on ; but, on the contrary, that he had

* Published in the *Medical Times and Gazette* for April 3, 1852, and afterwards, in summary, in the *Edinburgh Essays*.

swamped the cases of pneumonia, etc., as commonly so called, in a host of the minor ills that flesh is heir to, and that, in virtue of certain resemblances or analogies, such as Homœopaths are too fond of following out (*similia similibus*), slight diseases were made to do duty in the returns for really severe diseases; just as in the entire hospital returns, when compared with those of other hospitals, slight and trivial aggregates of disease were set against severe aggregates, without the least consideration of the elements of which these aggregates were composed.

Observe, now, that no part of the reply here given to the perverted and untruthful use of these hospital statistics, is due to the spontaneous admissions of the homœopathic journals and authorities; who never hesitated to urge to the utmost, in all manner of times and places, and with every kind of hyperbolical exaggeration, these statistics against those of ordinary medical treatment, till their force was broken by the doubts thrown upon them—in the first instance, by Dr. Balfour and others, who had personally observed the Vienna practice; and afterwards by myself and others, who had reasoned about it. Even after this, and to the present hour, homœopathic writers find it convenient to ignore the reply, and trumpet forth, though in a subdued tone, the appeal to these documents as compared with ordinary practice. Dr. Henderson, indeed, avers that he knew all along the faithlessness of the comparison as regards the general statistics. I am sorry for him if it be so; for it was his clear duty in that case to have made, from the first, those admissions which he afterwards made when, from the course the argument had taken, he had no other way open to him. I believe, however, that Dr. Henderson did not clearly apprehend the matter at first, and that, though the general statistics may have looked suspicious to him, he trusted them too much for his own good; that, in fact, Dr. Fleischmann was a chief agent in converting him to Homœopathy; and hence the anxiety, I believe quite sincere on Dr. Henderson's part, to save Dr. Fleischmann's character.

But what are we to say, after this, of Dr. Fleischmann himself, who, amidst all the turmoil produced by his statistics, sits unmoved and calm as the Phidian Jove, and sees his

labours employed to undermine the reputation, and depreciate the results of medical practice, in a way which even Dr. Henderson now admits to be unfair and inexcusable; and this, without a word of disapproval or of explanation?

Dr. Henderson, and the homœopathic authorities generally, have not attempted to deal with any part of my argument (as given above in summary) except that which relates to pneumonia. And how do they deal with this part? By the assertion, that whatever may be the case in the rest of the world, pneumonia is really, by a strange exception to the common rule, a far more common disease *in Vienna* than bronchitis, or any other disease of the chest singly; and, along with pleuritis, of greater frequency than all the rest put together. For, unless the defence goes thus far, it is of no use for its purpose of propping up Dr. Fleischmann's statistics.

Now, let us see what are the facts. I had already stated most of them by anticipation; for it was easy enough to foresee what would be the defence. In the General Hospital of Vienna (not homœopathic), there were received in the year 1848 the large number of

21,409 cases of disease. Of these there were

1314 tubercular phthisis;

2078 catarrh, *including*, as Dr. Henderson will admit, bronchitis;

427 pleuritis;

509 pneumonia.

In the Homœopathic Hospital of Vienna, there was, over several years, an aggregate of

6501 cases; and among these there were

98 of phthisis;

118 of catarrhs, including bronchitis, cough, etc.;

224 of pleuritis;

300 of pneumonia. Let us throw into the list of bronchitis, if Dr. Henderson will, any reasonable proportion of the cases of influenza, which he thinks unfairly excluded; still, the cypher of pleuritis and pneumonia will enormously exceed the Viennese average, as shewn in the General Hospital, whether as compared with the total number of cases, or with phthisis, or with catarrh; as any body may

see with a very slight exertion of that “common sense,” which Dr. Henderson so particularly extols in a question of this kind.

I now venture to assert again what I asserted in 1852 ; and what, though it may seem unpleasant, is nevertheless true, and necessary to be said in no mincing drawing-room phrases, but in the language common to all honest men, when injured truth calls for redress. I assert that Dr. Fleischmann *did* treat “miniature types” of disease, and called them by formidable names ; that he *did* sedulously exclude from his hospital incurable cases, except in such proportion as gave a colour of non-exclusiveness to the returns ; that he *did* admit, in enormous proportion, trivial cases, for which he could devise no other than trivial names ; that the mortality of his hospital, these things considered, instead of being small *was large*, perhaps very large, as compared with that of other hospitals ; that the mortality of acute diseases in his hands *cannot* fairly be compared with their mortality in other hands ; nor could Œdipus himself solve the riddle, what the proportion of that mortality really was, unless the cases themselves had been watched and numbered by eyes far more experienced, and minds far less biassed, than any that were engaged in the work.

I wrote in 1852 as follows :—“If good faith, sound judgment, and mature experience, be not at the root of an appeal to my convictions, the mere accumulation of instances, and their apparently exact statement in numbers, appears to me only the statement of cumulative error, in which the bias, the blunders, the dishonesty of the recorder may be assumed to be concealed by the difficulty of removing the rubbish he has collected around them. It is enough with respect to homœopathic statistics, to know that they are collected by a few obscure hospital physicians, from cases selected, named, and treated without control, with the single object of procuring facts in aid of a preconceived therapeutical dogma, and with the knowledge that the organs of quackery are prepared to carry the results all over Europe to the public ear, as an indubitable triumph.” This was hitting hard, I admit ; but I maintain that I both required, and had a right, to hit hard ; and the best proof both of the necessity and the right, is the deplorable fact that homœopathic hospital statistics are still

appealed to in 1857, while arguments, such as those I have given above, are passed over in silence. Nevertheless, my former hard hits did good service ; and, therefore, I do not grudge repeating them on the present occasion. But to return to the defence.

The only witness in exculpation called by Dr. Henderson, is one about whose evidence I should have desired to say nothing ; not surely, because that evidence helps Dr. Fleischmann, but because the statement of my honest opinion must bear the appearance of incredulity as regards some of the conclusions of the witness in question, my excellent friend Dr. Balfour of Cramond. I am quite sure, however, that in the interest of truth he will pardon my saying the little that I find it necessary to say on this subject. Dr. Balfour went abroad when a very young man, just escaped from the schools. I believe he had had no better opportunities of clinical study than the ordinary run of students of that day had before graduation ; and these, as I know well, were not great. Dr. Balfour carried a commission from Dr. (now Sir John) Forbes, to examine into the practice pursued in the Homœopathic Hospital at Vienna, the statistical results of which had by this time been blazed about all over Europe by the homœopathic journals as a marvel of successful treatment. Dr. Forbes was, as is well known, predisposed to believe in these statistics up to a certain point, not from faith in Homœopathy, but from distrust in physic ; a consideration which is, however, of no further importance to the present inquiry, than as it explains the singularly easy credulity with which he allowed himself to be seduced into admissions that have ever since been the natural food of Homœopathy ; and on which, truth to say, both the *British Journal* and Dr. Henderson thrive and waxed fat, so that they seem to be never tired of chewing, over and over again, the cud of the delightful morsel. Dr. Balfour executed his task, I doubt not, with the ability, honesty, and courage which are his characteristics ; the only error he made was in accepting such a commission at all ; and especially in accepting it before his experience had been enlarged, and his mind and character strengthened by contact with poor human nature under its various aspects of physical disease, moral imperfection, and intellectual crot-

chettiness. Conceive now the position of this youthful "Daniel come to judgment!" That he should hesitate to state anything tending to invalidate the homœopathic data, except upon the clearest and most irrefragable evidence, was a necessary consequence of that position. That he would be puzzled and misled, overruled by authority, and overwhelmed by the responsibility of his position, being bound to give Dr. Fleischmann the benefit of every doubt, and yet profoundly conscious that he was, after all was done, to be decried as a monster of iniquity by the whole chorus of homœopathic claqueurs, was written in the book of fate before he started on his journey; and therefore he should never have started at all. I am quite sure that I best serve the cause of truth by refraining from any observations upon Dr. Balfour's evidence in detail; and by simply asking those that have any remaining doubts upon the subject of Dr. Fleischmann's statistics, to read that evidence through and through,* with these considerations in their minds; and to ask themselves whether enough does not appear, even in Dr. Balfour's too lenient pages, to justify everything that I have written above. Dr. Balfour, no doubt, saw cases of pneumonia during his stay, and contracted the opinion that pneumonia was a very common disease at Vienna; but whether Dr. Fleischmann's cases of pneumonia, and of the other acute diseases so triumphantly put forward, were fair examples of the diseases in question, as commonly treated in hospitals, is another and a very different point, which Dr. Balfour was probably not well qualified to decide at the time he tried it; and which, even had he been qualified, it was very difficult, if not impossible, for him to decide in the circumstances in which he tried it. Dr. Henderson may, if he chooses, accept Dr. Balfour's evidence only when it suits his purpose, and reject it whenever it tells against Homœopathy; but no one

* See *British and Foreign Medical Review*, vols. 22 and 23. I make only one extract, because it relates to a question of fact, on which, I believe, Dr. Balfour has not been contradicted:—"The whole process of the admission and discharge of patients is mysterious. Still, so much is certain, that most of those admitted have *previously* been visited at their own houses by the assistant. Many cases not improving, or apparently not likely to improve, are got rid of very summarily." This is, indeed, the whole Iliad of homœopathic statistics in a nutshell.

who knows Dr. Balfour will do so, and least of all will I, who believe that his only faults in the inquiry were his rashness in undertaking it, and his chivalrous disposition to accord to the authorities of the hospital every credit which he could fairly give them.

Dr. Henderson speaks of my language in referring to this subject in 1852, as being “coarse and unmannerly;” and evidently hints that he treats me with great good humour and refinement in so saying. Be it so; that is matter of taste. It is more to the purpose to observe that my language in 1852 was, and is, unanswerable and unanswered; for no one that knows anything about the matter will be taken in by Dr. Henderson’s undignified device of representing my strictures as being “hitherto unnoticed,” because “undistinguished by the trump of fame.” Such insinuations do not disturb my peace of mind—first, because I never courted fame in connection with the homœopathic controversy, having endeavoured to offer my sacrifices to that fickle goddess on another altar than that of mere polemical scribbling; secondly, because Dr. Henderson knows very well that if, as he says, he never read my arguments before, it is only *in form* that he can say so; he read them all *in substance* in one of the chapters of Dr. Simpson’s book, which chapter he studiously answered in every detail in which it was answerable, although for reasons good he did not even attempt to controvert by argument one of my positions. I felt very much obliged to him at the time for not dragging me by name into that hot and very disagreeable contest. I was quite content with observing, that my facts and arguments had had their effect, though unacknowledged, in altering the whole course of the homœopathic defences. Fleischmann’s statistics, which Dr. Henderson vaunted in his first publication, and which probably converted him to Homœopathy, have been given up, in their original application, by common consent. It is felt to be, as I said, “a sham and a fraud” to bring forward these statistics, as in any respect parallel with other hospital statistics. It is felt that the original use of them constituted them “mendacious documents.” It is felt that “the facts of Professor Holloway are attested by more impartial witnesses;” and that “the balance-

sheet of Mr. Hudson had a far greater appearance of numerical exactness." It is felt that the "peddling experiments" of Homœopathy can no longer be trumpeted abroad after the old fashion, nor can they, without "amazing effrontery," be compared with the "great labours of pure beneficence, of which the general hospitals of this and other countries furnish examples." It is felt "that the homœopathic returns are not only void of triumph to the system, but that they cover it with disgrace." It is felt, even as regards acute diseases, that "common sense" is not *quite* "competent to decide which of two systems of treatment is the best." It is felt, finally, by Dr. Henderson himself, that those returns of mortality in acute diseases, which he at one time glorified as showing results "far beyond the reach of any other known method of treatment," have had their very existence as scientific facts compromised by the arguments which he affects to have ignored until now; which even now he does not attempt to answer, except in the single case of pneumonia; and which he answers, in the single case of pneumonia, only by calling Dr. Balfour to witness that pneumonia is a very common disease in Vienna; a fact, the bearing of which on the point at issue, I have clearly shown above. Dr. Henderson calls on me to apologise to Dr. Fleischmann. Might I not rather, speaking in the name of truth and soberness, call on Dr. Fleischmann, and on all who have abetted him, to apologise for the base calumnies to which the healing art and its professors have for years been subjected by those who have spoken under cover of these delusive statistics.

After all, however, I am not going to break the peace with Dr. Fleischmann, and much less with Dr. Henderson; and I here publicly say that I do not adopt, and I never thought of adopting, the "coarse and unmannerly" expression which the latter of these gentlemen insists on putting into my mouth. When a man is once fairly in the wrong; when the passions and the necessities of a sectarian position bind him to his errors; when he is committed to the extravagancies of an unscrupulous party, and impelled by the voice of public opinion to find evidence in behalf of a nonsensical creed; when he is so notable a person that he must either retreat publicly from an untenable position, or

advance over new absurdities and new blunders ; when self-love and the spirit of contradiction are roused, it is rarely that an appeal, however strong and direct, to reason, probability, or even consistency, can obtain a hearing. Such a man, whether his domain be science, politics, or religion, will do and will say strange things, without incurring the condemnation due to a wilful and spontaneous violation of the truth. We may extend this charitable consideration to many men whom we believe to be guilty of worse offences than are charged against the concocters of homœopathic statistics. But this does not absolve us from the duty of placing in the clearest possible light, and condemning in the strongest possible terms, the offences themselves. And therefore I have no hesitation in adhering to the language, unmannerly, though it may seem to Professor Henderson, of my article in the *Medical Times and Gazette* in 1852.

Before laying down my pen, allow me to direct your attention to two slips of your own. You ascribe to me, in page 285 of your criticism, the expression, “scoffs at pathology,” which you seem to think was directed against Professor Henderson. You are quite mistaken. I doubt if I even used the expression ; at least I cannot find it ; but most certainly I never used it with any such reference. You are equally wrong in supposing that I have any wish to see Dr. Henderson dispossessed of his chair in order that I may take his place. I am too well satisfied with my present position as a teacher to entertain any such aspirations. But had it been true, that the Chair of General Pathology was the goal of my ambition, your insinuation to that effect was scarcely worth the double deviation implied in a misquotation, and a foot-note to match ; it was, besides, neither a refined nor a mannerly way of disposing of my arguments ; which I now leave, with great confidence, to work their own work.

I am, Sir,

Your obedient Servant,

W. T. GAIRDNER.

EDINBURGH, 6th April 1857.